The Historical Geography of the Wealden Iron Industry. By M. C. Delany. (Historico-Geographical Monographs.) Pp. 62 + 3 maps. (London: Benn Brothers, Ltd., 1921.) 4*s.* 6*d.* net.

THIS is the first of a series of "historico-geographical" monographs published under the editorship of Prof. H. J. Fleure, which are to be essentially research monographs. It would greatly enhance the value of the series if in future numbers the matter were indexed or at least paragraphed with suitable headings. A single chapter of forty pages does not facilitate reference. As regards its matter, however, this short monograph is well done, although it is difficult to follow the distribution of the Wealden forest at different ages on the sketch maps provided. The author traces the iron industry in this part of England from its beginning, in Roman days or earlier, to its decline in the eighteenth century, when, as is well known, it could no longer compete with the more favourably located industry on the coal-fields. There appears to be a gap in the history of the industry for seven or eight centuries after Roman times; at any rate, the author has been unable to find evidence of its existence in that period. Some interesting details are given of the methods employed and the kind of iron work produced.

Letters to the Editor.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Precursors of Wireless Telegraphy.

A TRADITION is growing (cf. NATURE, March 9, p. 316), and requires scrutiny, that it was owing to discouragement by Sir George Stokes that D. E. Hughes abandoned his experiments in 1879, anticipatory of the methods and apparatus of modern wireless telegraphy. This is in contrast to all that is known of Stokes' extreme caution in advancing opinions, for which in fact he has usually been blamed, for example by Kelvin and Rayleigh in connection with spectrum analysis. From the modest letter of Hughes published in the *Electrician* in 1899, and in Fahie's "History of Wireless Telegraphy," Appendix D, which describes his very remarkable investigations, such an inference could scarcely be fairly drawn: "The experiments shown were most successful, and at first they (Spottiswoode, Huxley, and Stokes) seemed astonished at the results; but towards the close of three hours' experiments Prof. Stokes said that all the results could be explained by known electro-magnetic induction effects, and therefore he could not accept my view of actual aëreal electric waves, unknown up to that time, but thought I had quite enough original matter to form a paper on the subject to be read at the Royal Society." Hughes continues that he was so dismayed at being unable to convince them that he actually refused to write a paper on the subject until he was better prepared to demonstrate the existence of these waves, and with this end in view he continued his experiments for some years (Fahie, *loc. cit.* p. 310).

The key to the matter has, I believe, been supplied by the extracts from Hughes' original notebooks, now in the British Museum, which Mr. Campbell Swinton read at the Jubilee Meeting of the Institution of Electrical Engineers, and which he has kindly shown to me in manuscript. They show that Hughes held that in some way "the effects were due to electric conduction through the air" (NATURE, *loc. cit.* p. 316). Those who knew Stokes would expect that he would demur stoutly to such a doctrine as misleading, and would insist that "they could be explained by known electro-magnetic induction effects." For it was not unknown even before Maxwell's theory (1860–64) that the inertia of such induction could propagate waves along wires, which, if of very high frequency, would travel, as Kirchhoff showed in 1857, with the speed of light. The transcendent advance of Maxwell's definite theory, confirmed as fact by Hertz in 1886–1888, was that in favourable conditions such waves could release themselves from the matter and travel free across space; and, more fundamental still, that it is just by such free transmission that all electric and optical effects become established.

It seems clear to my mind that the affair was a misunderstanding, such as can readily be imagined, between the tenacity of the practical inventor and the insight of the theorist who was conscientiously determined not to give countenance to a misapprehension of the nature of the phenomena. But if Maxwell had been present (he had recently died), or Kelvin, who were more closely interested in the problem of the nature of the transmission of electric influence than Stokes, they would perhaps have used further efforts not to allow the subject to drop; though it would at that time have required all the resources of theory to make progress along the lines of these experiments.

The episode is so interesting from the point of view of the philosophy of history of scientific discovery, not to mention the practical application of the microphone operating by loose contacts by Hughes himself, in the manner developed much later by Branly and Lodge and Marconi, that a full statement from all aspects should be on record.

JOSEPH LARMOR.

Cambridge, March 18.

Stonehenge: Concerning the Four Stations.

JUST within the surrounding earthwork of Stonehenge there are two stones symmetrically placed with reference to each other on opposite sides of the centre. There are also two mounds in corresponding complementary (or reversed) positions. The arrangement is shown on the accompanying plan (Fig. 1), the dimensions for which have been taken from Flinders Petrie's very careful measurements as published in his work, "Stonehenge — Plans, Description and Theories" (1880).

For the purpose of this paper the arrangement is referred to as "The Four Stations." The two stones are numbered respectively (on Petrie's system) 91 and 93, and the two mounds 92 and 94.

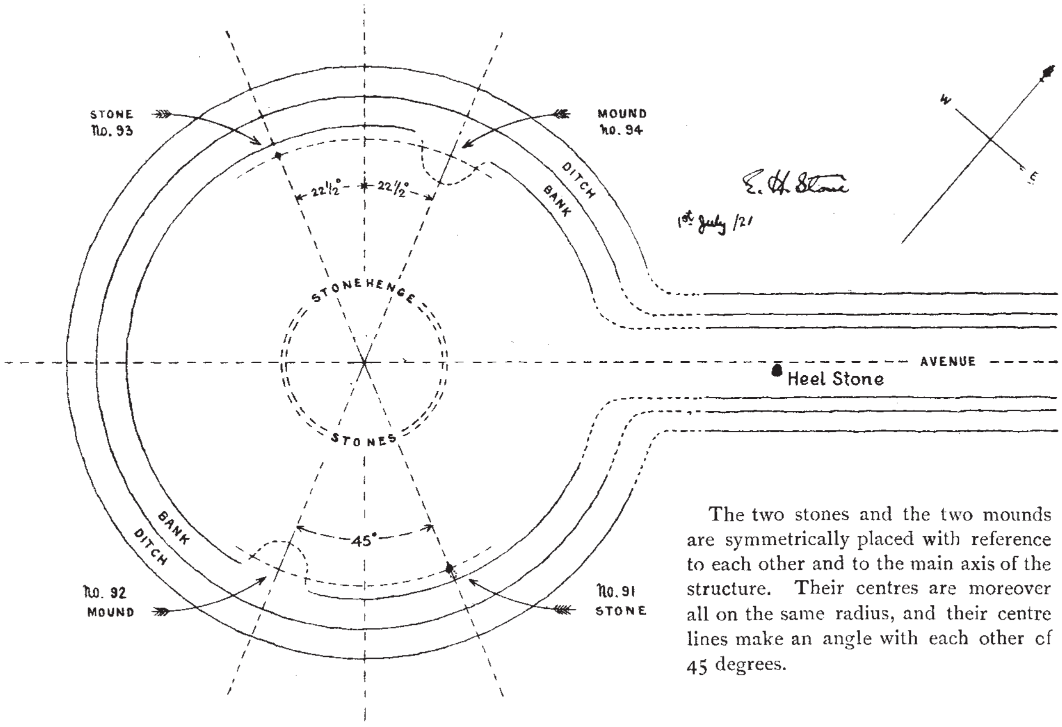
Concerning this pair of stones and pair of mounds Colt Hoare remarks :—

> "There are two small stones within the *vallum*, and adjoining it, whose uses have never been satisfactorily defined. The one on the south-east side is near nine feet high, and has fallen from its base backwards on the *vallum*; the other, on the north-west side, is not quite four feet high; both rude and unhewn. There are also two small *tumuli* ditched round, so as to resemble excavations, adjoining the *agger*; they are very slightly elevated above the surface, and deserve particular notice, as they may give rise to some curious and not improbable conjectures" ("Ancient Wilts," i. p. 144).

Colt Hoare opened the northern mound (No. 94), and in it found "a simple interment of burned bones." He also opened the southern mound (No. 92), "but found nothing in it" ("Ancient Wilts, i. pp. 144 and 145).

From the fact that a cremated interment has been found in mound No. 94 it has been assumed that both the mounds are barrows, and that they are moreover of the same period as the Round Barrows in the neighbourhood. On this assumption it is concluded that Stonehenge was constructed during the Bronze Age period or perhaps later. The arguments for this conclusion may be briefly summarised as follows :—

(*a*) In mound No. 94 was found a cremated interment.



The two stones and the two mounds are symmetrically placed with reference to each other and to the main axis of the structure. Their centres are moreover all on the same radius, and their centre lines make an angle with each other of 45 degrees.

FIG. 1.—Plan of Stonehenge. Scale—120 feet to 1 inch.

The two mounds are therefore barrows, and are of Bronze Age date.

(*b*) As barrows they would originally have been isolated constructions of circular bowl-shaped form. This [supposed] original form has been partly infringed upon by the bank of the main surrounding earthwork.

The circular earthwork surrounding Stonehenge is therefore of later date than the mounds, and is therefore also of later date than the neighbouring Round Barrows.

(*c*) The Stonehenge stone structure was erected after the surrounding earthwork.

Stonehenge is therefore of later date than the neighbouring Round Barrows, and was probably constructed near the end of the Bronze Age or perhaps later.

By some archæologists this argument is considered absolutely conclusive. Rice Holmes, for example, remarks :

"The stones were certainly not standing when Round Barrows were first erected on Salisbury Plain ; for one is contained within the *vallum*, which, moreover, encroaches on another" ("Ancient Britain," p. 476).

It will be observed that the whole of this argument is based on the assumption that mound No. 94 is really a Bronze Age Barrow. The mere fact that in it was found a cremated interment is, however, inconclusive, as we know that the Round Barrow people had a cuckoo-like habit of depositing a cremation in an existing hole or position originally intended for some other purpose. Col. Hawley's recent discoveries in connection with the "Aubrey Holes" furnish examples of this practice.

On Colt Hoare's plan the positions of the two stones and of the two mounds are not correctly shown, and from that plan it would not appear that they had any particular relation to each other or to the general scheme of Stonehenge.

But if the carefully made measurements by Flinders Petrie be correctly plotted to a large scale some very significant facts concerning these Four Stations at once become apparent, *e.g.* :—

(*a*) The four positions (Nos. 91, 93, 92, 94) are absolutely symmetrical in reference to each other and to the general plan of Stonehenge.

(*b*) They are all four on the same circle, *i.e.* their centres are all at the same distance from the centre of the structure.

(*c*) If a line be drawn through the centres of the two stones (91 and 93), and another line be drawn through the centres of the two mounds (92 and 94), these lines will intersect at the centre of Stonehenge.

(*d*) The two lines drawn as specified under (*c*) are at an angle of 45° (or an eighth of a circle) with each other. They moreover make equal angles of $22\frac{1}{2}°$ (or a sixteenth of a circle) with the cross centre line of Stonehenge.

This symmetry is very striking, and is so complete that it cannot be accounted for as a mere coincidence. It obviously points to the conclusion that the Four Stations, Nos. 91 to 94, were all specially located in relation to one another as parts of one scheme to serve some definite purpose in the general design of Stonehenge.

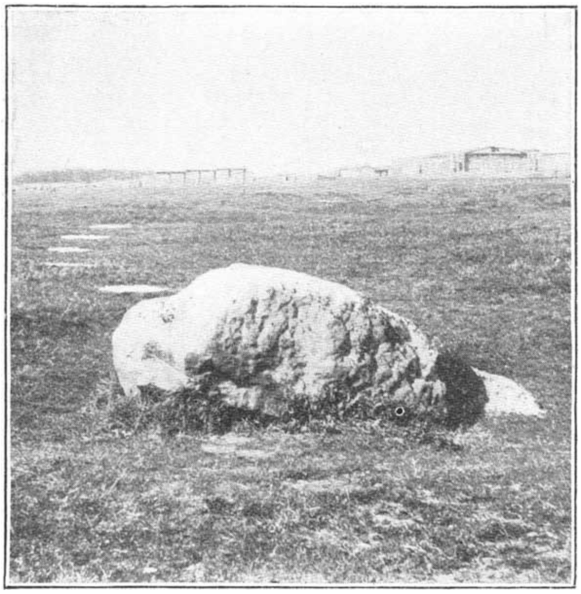


Fig. 2.—Stone No. 91. To south-east.

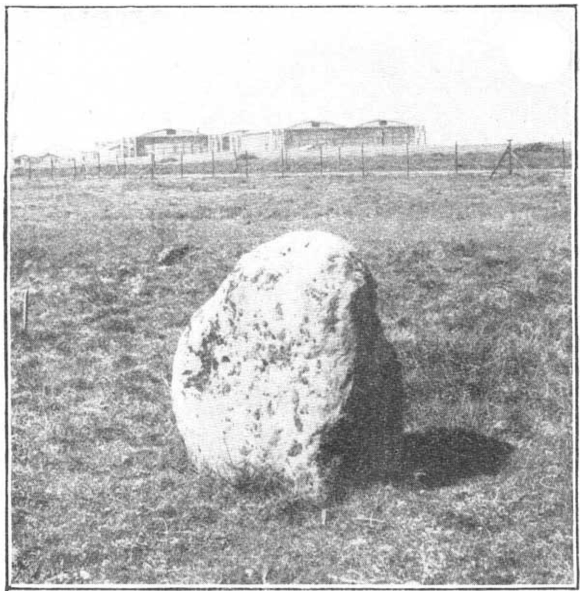


Fig. 3.—Stone No. 93. To north-west.

On this Flinders Petrie remarks :—

"On examining the stones and mounds 91 to 94 on the earth bank it will be seen that they are exactly opposite, stone to stone, and mound to mound. This strongly shows that they are contemporaneous ; as is also shown by the fact that the diameters joining their centres cross each other at . . . just half a right angle ; and further the diameters are complementary to each other, being symmetrical about the axis of the structure" ("Stonehenge," p. 21).

We cannot doubt that at one time there was a stone in each of the positions now indicated by the two mounds ; and that, whatever the purpose of the arrangement may have been, it had nothing to do with the neighbouring Round Barrows.

The two stones (Nos. 91 and 93), now in place, are shown in the accompanying photographs (Figs. 2 and 3). It will be observed that the ground is level around the base of each stone.

The two mounds (Nos. 92 and 94) are of very slight elevation, and are scarcely noticeable on the ground. Assuming that each of these two sites had at one time been occupied by a stone, we may suppose that the small amount of earth forming the present mound was thrown out of the excavation made when the stone was removed. When, later on, the cremated interment was buried, the incipient mound was perhaps trimmed up and added to.

It may be considered certain that the Four Stations were in no way connected with the "Aubrey Holes," and that they belong to a different period of Stonehenge history.

E. Herbert Stone.

The Retreat, Devizes.

Improvement of Visibility of Distant Objects.

In connection with the subject of some recent letters in Nature on "A Method of Improving Visibility of Distant Objects" by Prof. C. V. Raman (October 20, 1921) and by Mr. A. G. Lowndes and Sir David Wilson-Barker (November 10, 1921), it may be of interest to mention that two years ago I published a complete essay on the same question in the French Bulletin Officiel de la Direction des Recherches Scientifiques et industrielles du Ministère de l'Instruction Publique, No. 4, February, 1920, pp. 229-48, under the title "Sur l'utilité de la lumière polarisée dans les observations faites en mer ou au bord de la mer, et sur une jumelle à polariseurs."

Every advantage of polarised light mentioned by your correspondents, such as improvement of optical contrasts, visibility of colours in distant objects, etc., was considered and discussed in detail in that paper. I must mention that I took the research in hand in 1916 for military purposes, in connection with the French Ministry of Invention and Research and with the French Admiralty. The results and my former reports were communicated to the English Board of Invention and Research (1917). A little later, Prof. W. F. Durand, of Leland Stanford University, then the Scientific Attaché to the American Embassy in Paris, having been kind enough to order the translation of my reports into English, that English version was given likewise to the official agent of the British Ministry of Munitions Optical Department, Mr. F. C. Dannatt, now representative of the British Scientific Apparatus Manufacturers, Ltd., in Paris. At the end of the war the French Navy had a small number of binoculars equipped with spar polarisers of the Glazebrook-Ahrens type cemented with a special oil, as described in my paper. The constructor, M. A. Jobin, member of the French Bureau des Longitudes, supplied a few of those binoculars, at the request of the British Government, through Mr. Dannatt.

I do not wish to take up the space available in Nature with a translation of my paper, but a short summary of a few of my conclusions may be of interest.

Contrary to Mr. Lowndes's opinion, I objected very